Interactive comment on “Biogenic nitrogen gas production at the oxic-anoxic interface in the Cariaco Basin, Venezuela” by E. Montes et al.

Anonymous Referee #2

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General comments:
Montes et al. used measurements of excess nitrogen gas above atmospheric equilibrium to investigate N loss in the Cariaco Basin. They concluded that the excess N2 varied seasonally in response to changes in POC/PON export. The authors also found the excess N2 was comparable to the DIN deficit, which substantiated the hypothesis that the N2 excess is mainly of biological origin. Overall this manuscript is well written and suitable for publication after revisions.

Specific comments:
Section 3.2: In the introduction the authors state that waters were sulfidic below \( \sim 250 \text{m} \) which is also where most of the N2/Ar measurements were taken. They do not specify in the method whether sulfide was removed, corrected for, or did not affect N2/Ar. Granted, one would not expect good agreement between the excess N2 and DIN deficit if this were a major problem, however interfering gases are a well-known problem experienced by other researchers using IRMS (e.g. Emerson et al. 1999). For the information of other researchers please specify.

pp. 10559, line 13; pp. 10560, lines 3, 17. “Negative anomalies in N2...” “...undersaturated in dissolved N2”. It is misleading to state that N2 is undersaturated due to heating. The author’s sufficiently explain how, due to the differing solubility curves of N2 and Ar, heating coupled to incomplete equilibration can give rise to a normalized N2/Ar < 1, and thus a negative excess N2. However, heating sans equilibration would lead to a supersaturation of the absolute concentration of N2 gas. For instance, pp. 10559, line 13 should more accurately state “Negative anomalies in excess N2...”, or pp. 10560, line 16-17 “...in situ heating is the most likely cause for the negative values of excess N2...”

pp. 10562, lines 13 – 17. “We did not observe a difference...higher in March 2009 and September 2008”. It hard to reconcile the two statements that 1) there was no difference between March 2009 and September 2008 in the average nutrient concentrations of the stations sampled for excess N2, and 2) DIN was significantly higher between the two seasons at the CARIACO Time-Series station, especially given that the Time-Series station is one of the stations at which excess N2 was determined. This apparent incongruity seems to arise from the fact that the authors averaged all the nutrient data from all 6 of the stations sampled for N2/Ar. It would help if the authors showed the Time-Series nutrient data that was used to calculate the DIN deficit on Figures 4 and 9 (include data with O2 < 65 umol/kg) given that subsequent analyses and conclusions rely on this data from the Time-Series.

pp. 10564, section 5.3. I am surprised that the excess N2 matched the DIN deficit because: 1) The authors did not subtract the preformed normalized N2/Ar from the measured value (Devol et al., 2006; Chang et al. 2010). Normalized N2/Ar > 1 due to
physical processes have been observed at BATS, which this study is using as its source waters. This requires more discussion given previous work using normalized N2/Ar. 2) DIN deficits have been difficult to determine in anoxic waters due PO43- reacting with reduced metals, which must also happen in the Cariaco Basin. How did the authors determine this did not occur? As I already commented above, it would remarkable for the DIN deficit to match the excess N2 if the above issues were significant, however I do believe they warrant consideration.

pp. 10566, lines 8 – 10. “…where 0.86 is a factor that removed the effect of SRP production from organic matter…” The factor of 0.86 was used in the past when it was unknown if organic N remineralized during denitrification was converted to N2 and thus the loss of fixed N was simply a NO3- deficit. However, the authors are assuming the anammox process occurs and so the factor of 0.86 is unnecessary.

Figure 5. In many of the other figures the authors present average values for each season (nutrients, N2 excess, DIN deficit). They should be consistent and present the average for each season in this figure.

Figure 7. What is the variability of the POC/PON flux? Please add error bars to this plot or indicate range of values.

Figure 8. This schematic is not useful and should either be removed or significantly altered. First, there are undefined abbreviations (EZ, SUW). Secondly, other than changing the width of the arrows to represent increased fluxes, the authors make no attempt to illustrate the other differences they describe in the text between “upwelling” and “relaxation”. In the text they hypothesize that an intrusion of water from the Caribbean Sea could lead to a deepening of the oxic-anoxic interface and higher O2 in the upper water column, neither of which are represented in the schematic. In fact, contrary to the text, they show NO3- rich (and O2 laden) water intruding directly into the zone of N-loss, which, if this were actually occurring, would DECREASE anaerobic N loss due to the introduction of O2.